THE

BOSTON MEDICAL AND SURGICAL JOURNAL.

VOL. LXV. THURSDAY, NOVEMBER 28, 1861. No. 17.

TWO CASES OF LEUCOCYTHEMIA, IN WHICH CRYSTALS FORMED IN THE BLOOD AFTER ITS REMOVAL FROM THE BODY.

[Read before the Boston Society for Medical Improvement, November 11th, 1861, by CALVIN ELLIS, M.D., and communicated for the Boston Medical and Surgical Journal.]

IN THE BOSTON MEDICAL AND SURGICAL JOURNAL for Feb. 2d and 9th, 1860, there was reported a case of leucocythemia, in which crystals were found in the blood, and described by Dr. J. C. White. These were not noticed by Dr. Ellis at the time of the examination, and only several days after by Dr. White. It was then suspected that they might not form until some time after death, and this view has been confirmed by the following cases.

The first was that of a woman, 55 years of age, who had always been an invalid, subject to debility, dyspepsia and neuralgia. During the last year of her life she was under the care of Dr. Storer, and complained mostly of pain and fulness in the abdomen. There was a feeling of resistance in both hypochondria, and the liver was thought to be enlarged. She was feeble and anæmic, but never had hæmorrhage. She never menstruated, nor had any feelings which attend menstruation. Death was apparently caused by exhaustion, and occurred in May.

On examination, the heart was found filled with soft, recent, maroon-colored coagulum, resembling thick paint. This, under the microscope, was seen to contain a very large number of white corpuscles, with peculiar nuclei, like those described by Bennett and Virchow in connection with leucocythemia.

As the blood resembled that in which crystals had been previously found, it was preserved, in order to ascertain whether they might not form. It was not again examined until July 11th, when crystals were found in large numbers. The report of Dr. White in regard to them may be found at the close of the second case.

The spleen was three or four times as large as usual. The liver was also enlarged. The uterus was small, and its cavity obstructed just above the neck. Ovaries small. Other organs sufficiently healthy.

The second example was furnished by a remarkably strong, muscular man, a provision dealer, 24 years of age. About eighteen months before his death he received a severe blow in the epigastrium. When first seen by Dr. Dupee, six months after, in the summer of 1860, he was suffering from dyspepsia, and, after a few weeks, went to Maine, but returned to Boston and attended to his business during the autumn and winter. Two months before his death he was seen by Dr. Twitchell, of Keene, who discovered a tumor in the left hypochondrium, extending downward into the abdomen, but there was in the latter neither pain nor tenderness. Complained much of pain in the head, dizziness, and, occasionally, epistaxis. Anæmic, but not dropsical. No enlargement of the lymphatic glands was ever noticed. Some cough during the winter. Appetite "ravenous." Bowels regular. Without manifesting any other peculiar symptoms, he gradually lost flesh and strength, until death took place, July 10th, 1861.

Autopsy, five hours after death. The face was quite livid. Abdomen full and firm. The head was not examined. The pulmonary tissue appeared healthy, but, from the divided bloodvessels, there escaped thick, purplish or maroon-colored blood, like that which filled the cavities of the heart. Its color and consistence were those of thick paint, or the pulp of a softened spleen. The heart itself was quite large, but in other respects normal.

The liver was of a dark-brown color, and very large, weighing 13½ pounds. The blood was of the same character as that previously described.

The spleen was very large, and weighed 9 pounds. The edge was deeply fissured, and some portions of the capsule were white, firm and thick. On incision, the cut surface presented the usual appearance.

The kidneys were very large, each being about six inches in length. The lumbar and mesenteric glands were somewhat enlarged. No change of Peyer's patches or the solitary glands.

On microscopic examination of the blood, it appeared to be composed almost entirely of white granular corpuscles, in some of which a nucleus could be seen, and after the addition of acetic acid nuclei appeared in others. On the following day, another examination was made, at 9 o'clock, A.M., with precisely the same result, but, at a later hour, Mr. Gibson, one of the surgical house-pupils at the Hospital, noticed crystals like those previously described. It now appearing very evident that the formation of these took place some time after death, the blood was sent to Dr. J. C. White, together with that of the previous case, then very offensive. Dr. White furnished the following report:—

The specimens of leukämic blood were sent but a few days before I left town. The hasty examination I was then able to make assured me that it was chemically and physically similar to a specimen I received from you in 1859, and the peculiar crystals, which I then described and called leukosin, were observed to be largely present. In addition to these, other crystalline bodies were noticed in the blood, which was still comparatively fresh, such as I had never before seen, in the form of rosettes and of a reddish brown color.

On my return, the blood had become very putrid, and the forms last alluded to, together with the red and white corpuscles, had unfortunately disappeared. The leukosin, however, was unchanged, and indeed in a specimen of the blood, preserved by me, in which it was first discovered (now nearly three years ago), the crystals are to-day as sharp in outline as when first seen. I have nothing to add to what was at that time published in regard to their analysis. Since then they have been observed in leukämic blood in Paris, and a description of them, given by MM. Charcot and Vulpian in the *Gazette Hebdomadaire de Médecine et de Chirurgie*, 1860, proves their complete identity with those in our own cases.

Their constant occurrence in the blood of every case of leukämia observed here since their discovery, seems to justify us in the conclusion that they are really characteristic of the disease, and may yet give us some insight into its true nature. It is probable that they do not exist ready formed in the blood, so long as it is of the temperature of the body and in motion within its vessels; and several hours have elapsed, I believe in every case, after its removal, before they have been observed. An analogous phenomenon is noticed in connection with the separation of crystals of uric acid and oxalate of lime from the urine. The microscope seldom reveals their presence in this fluid when first passed, and it is not until some but little understood chemical change or ferment has taken place in it, that the crystalline forms of these bodies are separated and become visible. Reasoning from analogy, then, it is fair to suppose that the crystals of leukosin, although not circulating as such in the vascular system during life, still exist in some soluble form, and require but some change in the mutual relation of the other elements of their mother fluid after death, perhaps a lower temperature only, to assume their definite shape. Very truly yours,

James C. White.

The statement made by Dr. White, that these crystals have been found in every case of leucocythemia observed here since their discovery, is perfectly correct, but it should be understood that the blood in all these instances was of a uniform dark purple or maroon color, of the consistence of thick paint, and, except in the first case, showed no disposition to separate into yellow or purulent looking and dark-red portions. In this instance, even the same elements were found in the light-colored as in the dark portions of the blood.

SECRETION AND USES OF THE BILE.

By W. G. Bruce, A.M., M.D.

[Communicated for the Boston Medical and Surgical Journal.]

Before the venous blood from the chylopoietic viscera is allowed to reach the heart, it passes through the liver, where a fluid, rich in carbon, is eliminated from it, called *bile*, which is poured into the duodenum. Many circumstances have contributed to make the bile hold a higher position in the process of digestion than I think it entitled to, as it has been supposed that the sole office of the liver was the secretion of bile, and the chief function of this, when secreted, must necessarily be to take a part in digestion proportionate to the size of the organ furnishing it.

But the investigations of many modern physiologists establish, beyond doubt, that there are other and more important actions performed by the liver than the manufacture of bile, all of which tend to diminish the high estimation in which this fluid has been held as regards the process of intestinal digestion, either as a solvent, or as a mere excretor of noxious materials.

Various attempts have been made to estimate the quantity of bile secreted in a given time, under different circumstances as well as in different animals, with very varied results, and in truth we have no satisfactory knowledge on this point. In order to answer this question, fistulous openings have been made into the gall-bladder or ducts of various animals; and while all communication with the intestine was cut off by a ligature, the operator was enabled to collect the secretion at leisure. M. Blondot kept a dog in this condition five years, and many of those used by Drs. Bidder and Schmidt were killed after a two months' observation, while still in a viable state. The quantity which M. Blondot obtained in twenty-four hours from one of his dogs, was only 12½ drachms, and assuming from the similarity of the food of the human species and that of the dog, that a proportionate amount of bile would be secreted for the weight of the body, he concluded that the average quantity poured into the human intestines in twenty-four hours, was from six to eight ounces. The investigations of others, however, have been much more satisfactory. Drs. Bidder and Schmidt performed a series of experiments upon a large number of animals under a variety of circumstances—during fasting, during repletion, on full diet, on low diet—and they found that the average amount secreted by the dog in twenty-four hours, to be from twelve to fourteen ounces. On this estimate, we secrete daily from three to four pounds, which is probably much nearer the amount than the estimate of M. Blondot, and it is altogether probable that even this is rather below than above the mark. It appears, also, from these experiments, that no relation existed between the size of the liver and the amount secreted. It might have been supposed that the greater the weight of the organ, the more productive would it have been; but, on the contrary, it was found that in the sheep experimented upon, the *smallest* livers gave the *greatest* quantity of bile, and in the cats quite regular ratios were obtained.

The bile is secreted from the blood of the vena porta; as the small size of the hepatic artery, compared with the large size of the gland, and the small degree in which it can participate in the formation of the extensive capillary plexus of the organ, evidently disqualify its contributing to the secreting process.

Bile is *first* found as a separated product in the minute canals or bile ducts. It is a thick, ropy fluid, of a greenish-yellow color, an acrid bitter taste, and a peculiar nauseous smell, with a specific gravity of 1.026 to 1.030. Its secretion appears to be continuous, but it is augmented or diminished according to the stage of the digestion. It attains its maximum ten or twelve hours subsequent to a hearty meal; and from then until twenty-four hours after the meal, it gradully diminishes until it reaches its minimum, or the least quantity secreted, about two or three hours after eating. This is of the highest importance as regards the action of the bile in the process of digestion, for it is natural to conclude that it would be secreted in largest quantities at such times as it is most needed; and if its presence was necessary in the early part of the passage of the food through the digestive canal, it would be secreted in large quantities during active digestion. It however does not reach its maximum until digestion is nearly completed, or at least very far advanced, and hence it may be inferred that its main object is to subserve some secondary purpose, or rather to prevent any unnatural action which may be likely to obtain at that period.

It is found that the quantity is very much diminished or increased by the nature of the food, as the amount obtained from the cat, under a full flesh diet, was more than twice the quantity secreted while under an ordinary diet. Again, fatty food is found not only not to increase it, but the secretion materially diminishes under its use. It might be anticipated, that in the formation of a substance so rich in fat as the bile, it would be materially assisted by the copious use of fatty food; but experiments prove it otherwise, and it may be said that a diet of pure fat is, so far as regards the secretion of bile, analogous to a starving diet.

The color of the bile is dependent upon a variety of contingencies, as the greater concentration of the fluid bile in the gall-bladder, during retention in that organ, it oftentimes having been found to present from ten to twenty-five per cent. of solid matter, while the natural amount of solid matter in fresh-flowing bile is only from two to seven per cent. The peculiar quality of the food natural to the animal, causes a change in the color of the secretion; thus in carnivora it is of a yellow, ranging from a golden to a dark-yellow, and in herbivora it exhibits various hues of green. Another important agent in this action is the presence of oxygen. It has been ascertained that if a quantity of yellow bile be subjected to the action of oxygen, it very quickly becomes green; and if green bile be *deoxydized* by zinc, it again resumes its yellow color. Therefore it is very materially influenced by the amount of oxygen conveyed to the liver by the blood.

Bile, as it exists in the gall-bladder and intestines, is alkaline, probably from the admixture of a considerable quantity of mucus, as when that has been precipitated it is ascertained to be neutral, as is also the case when examined before the admixture of mucus. Acid bile is only formed by its partial decomposition, and hence it has been stated that fresh-flowing bile is neutral.

As regards the action of the bile upon the aliments or substances taken into the digestive canal, many conflicting opinions have been presented, and although each has been supported by what was supposed to be sufficient proof, yet each in its turn has been doomed to an overthrow by a more successful hypothesis which followed. It has been taken for granted that bile was in some way capable of dissolving the food, and thus assisting in digestion; but how, or upon what particular kinds of material it produced its effects, was not known, and in fact it is only within a very few years past that *any* proof existed as to its performing this action. The oldest hypothesis is that advanced by Boerhaave, and contended for by some physiologists of the present day, which attributed to the bile the power of saturating the acids of the chyme with its alkali, and, although that may be to a certain extent correct, yet it is found by experiment that the chyme of the intestine is still acid, after the bile has been mixed with it, and it is well known that a very small quantity of free acid mixed with the bile is sufficient to destroy its alkalinity. This, however, takes place in the following manner: the alkali of the bile, being in combination with the fatty and resinous acids, when thrown into contact with the stronger acids of the intestine, as the lactic, hydrochloric, &c., unites with them, setting the biliary acids free, which communicate an acid reaction to the intestinal contents or chyme, and in this regard may the bile contribute to the neutralization of the free acids contained in the chyme.

It has been observed by Dr. Bidder, that the bile has no power, or at least but very slight, of dissolving albumen, even after many days' maceration. The following experiment demonstrates how small is its solvent power upon that form of albumen found in cheese, known as *caseine*:—Three portions of cheese were digested for thirty-four hours at 100° Fahr., one in pure water, another in water and bile, and a third in water and glychocolate of soda, when it was found that the portion mixed in the water and bile lost only eleven per cent. of its weight, while the other two had lost seventeen per cent.; showing that instead of the bile being a good solvent, it has even less power than pure water.

Upon a microscopical examination of the fæces of a dog, fed entirely on flesh, and in whom the ductus choledochus was tied, thus preventing the entrance of bile, it was found that they contained no traces of muscular fibre, but it had all been digested as readily and as perfectly without as with bile.

By experiment out of the body, it is found that neither starch nor albuminous substances are essentially changed, even when digested for a long time with fresh bile.

Haller first ascribed to the bile the property of dissolving fat. It is well known that the intestinal villi have the power of taking up this substance in large quantities, and the question arises, whether it requires any modification previous to its passage, and if so, is the bile requisite to make that modification. This seems to be answered in the affirmative by several experiments, and it is affirmed by Frerich that the bile contributes, in association with the pancreatic juice, to the perfect disintegration of the fat, and thus to a great degree promotes its absorption. In a dog, in which the entrance of bile into the intestine was prevented by ligature, it was found that but a small quantity of fat was taken up, only about half an ounce during twenty-four hours; while a healthy, uninjured dog was, upon the same kind and quantity of food, found to absorb about ten times as much, or five ounces; which proves that although a small quantity of fat may be absorbed by the villi independently of the bile, yet its presence increased very materially its absorption, either by rendering it more susceptible of being taken up, or the membranes more suitable for its absorption.

Again, a very important action of bile on fleshy matters, which is of practical bearing as regards our health and comfort, is its *antiseptic* properties. It is found, in animals, that when no bile is allowed to flow into the intestines, the contents undergo rapid decomposition, accompanied with the secretion or formation of fœtid gases, and fæces smelling like carrion. The same condition is sometimes observed in patients with jaundice; and although it is not attended with any disastrous or fatal consequences, yet it is excessively distressing. In animals fed solely on starchy food, no decomposition took place, the fæces being sour rather than putrid. Hence, the bile acts on albuminous substances to prevent decomposition, and preserves them in that condition in which they may be the more readily absorbed by the villi of the intestines. Again, the *water* which is contained in the bile acts as a solvent upon the soluble portion of the chyme, rendering it capable of transmission through the membranes. This matter keeps up a continuous circulation through the liver and intestines, performing on its route a double office, according as it flows from the liver to the intestines, or from the intestines to the liver. In either case it passes a filter, which is permeable to only a particular class of substances.

Prout is of the opinion that bile acts upon the proteine compounds in a manner by which they are converted into albumen, a theory apparently supported by the experiment of Frerich, that when filtered chyle is mixed with a quantity of bile, it will coagulate on the application of heat. Prof. Lehmann, however, was unable to obtain a similar result, although he performed a number of like experiments.

Some physiologists assert that the bile is a mere excrement, or *used-up matter*, which is for the welfare of the body to be got rid of as soon as possible. This, however, is rendered untenable by the manner in which it is carried out. If it were simply effete material, it would evidently be taken from the body by the shortest possible course, and not be allowed to traverse such an extent of surface on which it would act as a foreign body, and thus occasion an unnecessary risk. The making of a biliary fistula would not only *not* prove an *injury*, but would be a decided *benefit*, to animals; but such is not found to be the case. That the bile is partly an excretion is probable, and this is undoubtedly true as regards all other secretions. It has been observed by Boerhaave, that "almost the whole mass of the fluid secreted by the liver, is again drunk up by the absorbing veins from the alimentary and fæcal contents before they arrive at the anus."

It may be asked, if the bile is so extensively absorbed by the blood, why is it that it is never found in that fluid, as, from the most careful examinations, no trace of re-absorbed bile could be detected either in the chyle or portal blood? Or why is it necessary that it be formed at all, as its short circuit from the liver to the intestines, and thence directly back into the liver, may at first sight seem objectless and superfluous?

As regards the *first*, we may say that the bile, soon after its passage into the intestines, undergoes chemical decomposition, a portion passing off as effete material, but the larger part, as its soluble salts and cholic acid, passing directly into the blood, not as *bile*, but as *constituents* of that fluid. But if we have regard to the necessity of its formation, or to the fact that it seems useless for a substance to be formed from the blood in order to be again taken up by that fluid, we can only reply that substances are repeatedly eliminated by the action of all the other glands of the body, and all taken up again by the vessels, and we are far from being able to comprehend the aims of nature or her mode of arrangement.

This we see is the case as regards chloride of sodium, iodide of potassium, the salts of ammonia, &c.; and the re-absorption of chloric acid is no more unnatural than that of any other material whose particular action and use are alike inexplicable.

The bile may be said to perform the following duties, which from the present state of our knowledge is all that can be attributed to its action:—

1. To remove a small amount of effete material in common with the action of all other secretions.
2. To prevent the too rapid decomposition of albuminous material.
3. To assist in the passage of oily substances through the intestinal membranes.

Barnstable, Mass., 1861.

STRICTURE OF THE COLON.

[Communicated for the Boston Medical and Surgical Journal.]

Messrs. Editors,—I send you the following case. Oct. 23d, 1861, I was called in consultation with Drs. Vance and Sanger. Patient was a man about 70 years of age; had been troubled with severe cough for several months previously. Had also had several attacks of severe colic—one of which, nearly terminated his life; but he finally recovered, and was apparently as well as before. When we arrived, we found him suffering severe lancinating pain in the region of the sixth and seventh ribs of the left side, about six inches from the median line; the ribs protruded, probably two inches beyond the natural plane, forming, apparently, a large tumor or abscess. Percussion produced the tympanitic sound, and auscultation developed a humid crackle. The pain was somewhat relieved by steady pressure. We were told that the swelling had appeared about twelve hours before we were summoned, but the result shows that such could not have been the fact. Auscultation over the right lung revealed almost complete hepatization; over the left, the respiratory murmur was distinct in the upper lobe. We ordered anodyne doses of sulphate of morphia, and left him, convinced that death must soon supervene. This took place about twenty-four hours after.

The autopsy developed firm adhesion of the right pleura to the ribs, also partial adhesions of the left. The substance of the lung was found completely infiltrated with miliary tubercle, but softening had not taken place. In the region of the tumor I have described, we found the diaphragm raised two or three inches from its normal position, pushing the apex of the lung upward and backward, by a coil of the transverse colon. It had the appearance of having been dragged along from the right side till it formed a sort of sigmoid flexure. In the centre of the flexure we found a stricture of the intestine, leaving a passage scarcely one half an inch in diameter, each side of it; the intestine was distended enormously by gas. The two orifices of the stomach were nearly vertical with each other; liver normal; other organs healthy.

Newark, Ill., Nov. 14th, 1861. H. C. Robbins, M.D.

A CASE OF HERMAPHRODITISM.

[Communicated for the Boston Medical and Surgical Journal.]

Messrs. Editors,—In the examination of recruits for the United States Naval Service, this morning, a very interesting specimen presented himself for admission. He combines, in a most remarkable manner, a perfect female form with male generative organs, though in a very embryotic condition. Perhaps no better idea can be conveyed of him, than by giving a general outline of his features.

He is a native of Ireland, and now 22 years of age. He has followed the sea for the last ten or twelve years. He is 5½ feet in height, with blue eyes, light hair, and light complexion. His usual weight is 150 pounds. His voice is of a decidedly feminine character. He has no beard, and no hair, except upon his head and a few short scattering appendages around his rudimentary penis. His hands, arms and shoulders have the form, plumpness, and in every way the appearance of a well-developed female. His breasts are full, round, soft, and beautiful, with well-developed nipple and areolæ, and identical with the breast of a virgin. The curves of the thigh, so characteristic of the female, are well shown upon this truly wonderful form. His legs, feet and body all conform to that of a female. Upon a superficial observation of the genital organs, they were found to resemble those of a female, with a greatly enlarged clitoris. But upon more minute examination, we find the apparent external labia to consist of two folds of a diminutive scrotum, placed in apposition to each other. The raphe is distinctly marked. The testicles, which are about the size of a large pea, are situated high up in the region of the external rings, a little in front, and beneath them. The penis is about one inch and a half in length, with corresponding circumference. The prepuce adheres to the glans penis. He represents that he is capable of coition with females, and that his feelings sometimes prompt him to indulge. His penis is capable of erection, and, he states, of emission of semen.

These, I believe, are the general outlines of an individual who to us is very interesting. Hoping that this brief communication may be of interest to some of your readers, I have thought it best to report the case. Yours, &c., J. W. Bragg,

U. S. Ship Ohio, Boston Harbor, *Ass't. Surg. U. S. N.*
Nov. 16*th*, 1861.

Bibliographical Notices.

Placenta Prævia; its History and Treatment. By William Read, M.D., Member of the Mass. Medical Society, of the Boston Medical Association, of the Boston Society for Medical Improvement, &c. &c. Philadelphia: J. B. Lippincott & Co. 1861. 8vo. Pp. 340.

Since Dr. Dalton's Physiology, we have seen no American medical book that has given us such satisfaction, nay, we should say which excites our pride so much, as the one now before us. And yet it is not a pleasant book; it is not one you can pick up and read and understand at will and for pastime. In fact, it is a book that one must take to as a study or not at all. It is what some would call "a dry book," and a good many "a heavy book," and it may be both without any disparagement to it. The subject of it does not fall within the few fanciful lines of medical science, nor does it present a field where the imagination has any right to play. Therefore, if the book be dry, it is properly so, and because the author has in the spread of the subject before him apparently felt he had too much fact to deploy to allow any room for rhetoric; and if it be heavy, it is because it is packed so full of fact. In truth, we cannot at this moment recall a monograph, even among those remarkable German ones (written generally in Latin), that shows more result of examination into the writings and opinions of others; a fairer exposition of these opinions in judiciously selected quotations; closer reasoning by the author himself upon them and upon his own observations, or a more concise exposition of his deductions.

These remarks, by all rule, ought to have been reserved for the close of our review, and have been used, as is usually the case, in winding it up; but what we have asserted so impressed itself upon us at the outset, and continued to enforce itself so much upon us as we advanced, that we could not avoid stating it.

The first Chapter, of 23 pages, is given to the medical history of the accident, in which the views upon it of the most prominent writers, from Hippocrates down, are concisely stated, and the various errors they fell into in attempting to explain the cause, and the precise nature of it, pointed out. In this we are sorry to see a charge brought against Dr. Robt. Lee of unfairness in his quotations from Mauriceau, while commenting upon the views of the latter. We must confess to a partiality for Dr. Lee, on account of the stand he has so firmly taken against some of the notions about uterine disease, by which specialists in this line have been of late so led away, but we have to confess that the charge of Dr. Read seems fairly sustained.

The next Chapter, we think, is wrongly headed "How Produced." This question is not here fairly entered into, nor could it be until the next Chapter, on the Physiology of the subject, is disposed of, and more particularly the series of changes in the development of the lower half of the uterus during pregnancy is explained. The simple gravitation of the ovum towards the *os uteri*, on account of the upright posture of the human female, can scarcely be called in itself a cause. Were it so, the accident should be more frequent, instead of occurring, as the author shows, only about once in 1200 cases.

Chap. III. is by far the most interesting of any in the book, if we consider any single one by itself. It is devoted to the physiology of the subject, and we cannot but wish that the author, instead of continuing here his conciseness of style, had given himself more space and more freedom and fulness of expression—more tether in every direction. This we consider absolutely necessary in entertaining the question of the nature of the union, or rather the method of adaptation of the placenta to the uterus. It will be at once apprehended by the reader, that this is not an outside or offside question, but one of immediate practical importance—a clear and truthful solution of which determines our resort in many cases to this or to that method of treatment, at a moment when the life of our patient depends upon prompt and proper action. It is one that must be answered before we can tell whence comes the hæmorrhage in placenta prævia, and at what time we ought to dread it and how we ought to meet it. To properly appreciate this question, and the amount of discussion that has grown out of it, we should follow the author through his various researches, and see the number of strong names he quotes on both sides—or rather, we should say, in favor of the different explanations that have been presented for its solution. We have not, we regret to say, room to do this. We can only give, as an earnest of what is afforded us by the labors of Dr. Read, that he quotes the opinions on this subject, of forty authors, besides referring to many others incidentally. Of all these, however, the one that interests us most both in himself and views individually, and in their relation, or, rather, in their opposition to Dr. Read's views, is Dr. Dalton. He is not only the last in the field of physiology, but his course through it, and his present stand upon it, has been and is now striking and honorable. Acquainting himself thoroughly with what has been hitherto attempted and done, examining carefully and minutely the labors of those who have gone before him, or are now at work around him, he has nevertheless not permitted any one to ease his shoulders of the burden, or his hand of labor, but has patiently set himself down to verify all he has been told and assured of by others, through the cunning of his own eyes, the sharpness of his own sight, and the clearness of his own brain. And yet, so far as placentæ and uteri and their adaptation to each other are concerned, Dr. Read seems to have gone to work as thoroughly, as free from bias, and with as much zeal and devotion, as Dr. Dalton, and still they differ in their results. We regret that space is not allowed us to lay the controverted points fully and fairly before the reader. He could only understand the subject by a full exposition of both sides—a statement of what is claimed by each, and the grounds, experimental or rational, upon which the claim is made. We must feel, however, that when two such men, equally zealous in the cause of science, come together, even in opposition, the true and real state of the case cannot much longer be left a controversy, in a matter of physical fact.

Chapter IV. is given to "Special Causes of Hæmorrhage," and under this head several very interesting questions are discussed, as more or less involved in the answer to the chief one:—The separation of the placenta from the uterus—the normal situation of the placenta upon the uterine walls—the period and rate of the progressive increase of the placenta, and the relative size and weight of the placenta to the child. Besides having, as we have just said, a value as a cause of hæmorrhage, and a mutual bearing upon each other in exhibiting such cause, each of these is in itself a physiological problem of much interest, and is described as befits it. The error of the prevailing notion that the normal position of the placenta is at the fundus of the uterus, is shown clearly by the instance of over 700 cases. In 100 of these, by Von Ritgen, the placenta was within two inches of the os uteri in 49, and Coombe shows it was "in the vicinity" in 96 out of 100. The relative weight of the placenta at different periods of fœtal life, and its comparative weight with that of the child, is illustrated by over 400 observations. The facts being thus carefully arrayed, 18 pages are devoted to an estimate of the proper value of them, in elucidating the question proposed.

The preliminary questions being disposed of, the treatment of placenta prævia is taken up. The subject is approached, as every other in the book has been, with a clear perception of its importance, and with a diligent search for and careful marshalling of facts bearing upon it. To give some idea of what the author has done under the latter head, we may state that he exhibits 956 cases, arranged in nine tables. Each of these tables is afterwards separated, analyzed, and the individual cases it contains classified under new characteristics and fresh aspects. The mechanical work, so to call it, of this part of the book is immense; the simple sorting out and arranging of the cases and the details connected with them—"pigeon-holing" each under its appropriate head and in its proper column—this by itself is no slight thing. But beyond this we find a very nice and most careful analysis, in which the true value of each case is ascertained, and the case set forth as an example or illustration of the treatment to be pursued under this or that modifying circumstance. Of course, it is wholly impossible to make a quotation here, and, without one or more, we cannot hope to give the reader a fair idea of this part of the work. The results, however, at which the author has arrived are given us in the last four pages, where they are classed under eleven heads. Of these we may note as most important the 2d, which says, "The danger to the mother is less when the os uteri is completely covered than when a portion only is involved in the attachment of the placenta; and least of all when the attachment becomes nearly or quite central with reference to the os."

The 3d looks to the influence of the *condition* of the mother. It is not the amount of blood lost, but how the woman bears it.

The 5th says, "The danger to the mother is materially increased by artificial delivery;" but, he adds, this is not from the operation so much as from the remedy being resorted to often *in extremis*. In view of this, the 6th conclusion advises resort to interference before exhaustion is attained. And this accords fully with what experience has taught us in our own practice, and in those cases we have been called to in consultation in this dreadful accident. The 9th paragraph, expressive of a prudent distrust of the tampon, except under certain circumstances, we are very glad to see. We have seen some fatalities, and known of several very narrow escapes, from misplaced confidence in this mode of treatment.

The last paragraph advises that, where there is great exhaustion, and turning is resorted to, "after the feet have been brought down, the body of the child should be left undelivered until the uterus has been aroused to contract." A most prudent precaution in using version under any necessity.

We have thus taken a brief survey of a book which we feel cannot fairly be judged in this way. Indeed, many might read it, and yet have very little idea of the labor bestowed upon it. It is not labor that shows, but our experience in the same line, though to a far less extent, enables us to appreciate the patience and perseverance, continued day after day and month after month, by which Dr. Read has succeeded in piling up around him, and for each purpose as he wanted it, such a mass of material, and then sorting out, analyzing and arranging it as it now presents itself to the reader.

The faults of the book—that is, the faults for one of our habits and liking in our reading—are those we have mentioned at the beginning of this notice. The author has striven, and with perfect success, to pack the fullest number of authorities and the largest amount of fact in the smallest possible space. To a certain extent this is highly commendable—a great virtue; and the errors of most medical writers we feel and are sorry to assert are far on the other side. Diffuseness, carelessness of expression, unsupported assertion and statement of unimportant fact, interwoven with highly unnecessary display of rhetoric, are much oftener fair causes of complaint against the monographs of the present day. And it is possibly on account of being obliged to accommodate ourselves to many such faults, that when finding one, the author of which discards all that is adventitious and ties us down to investigation and proof as rigidly bare as that of a mathematical proposition, we feel a desire to be eased up occasionally and helped along, and to have the road made pleasant to us. But we cannot complain greatly when the resemblance to the problem in mathematics is carried out by the result, as in the case of Dr. Read's book, where this is almost as conclusive and final. W. E. C.

Fiske Fund Prize Essay on the Morbid Effects of the Retention in the Blood of the Elements of the Urinary Secretion. By WILLIAM WALLACE MORLAND, M.D., Member of the Boston Society for Medical Improvement; one of the attending Surgeons at the Central Office of the Boston Dispensary, &c.; being the Dissertation to which the Fiske Fund Prize was awarded, July 11, 1860. Philadelphia: Blanchard & Lea. 1861. 8vo. pp. 83.

THIS is a treatise on a subject which has of late received much attention. The author, who had already achieved reputation by his work on the Urinary Organs, has shown his usual care and method in the management of his subject, and has given to the profession the best essay on this branch of pathology with which we are acquainted. It will be noticed, that among the great variety of constituents of which the urine consists, the number is extremely limited of those that seem to bear upon disease. In fact, urea and uric acid are the only ones, the retention of which, observation thus far appears to have connected with serious pathological changes. The question as to the *materies morbi* of rheumatism is here discussed, and the various opinions of the later pathologists ably summed up. It appears that while there are still those who incline to the lactic acid theory, there are others, and those among the more recent observers, who refer the disease to an excess of uric acid. Among the latter, are Drs. Barlow, Bennett, Copland and Thudichum. The present theory of Dr. Bennett with regard to gout and rheumatism, "that they are both connected with an increase of lithic acid in the blood, this acid being in the former dependent upon excess of the primary, and in the latter upon excess of the secondary digestion, is certainly a plausible one, but so far as it relates to rheumatism is not entirely satisfactory. As our author remarks, the pathology of this disease is by no means yet settled, and until it be, the relation it bears to gout must remain an unsolved problem.

On the subject of urea and uric acid, the essay is full and clear; and with regard to the other urinary constituents, everything bearing upon their relation to disease has been carefully considered. The obligations of the profession are due to Dr. Morland for publishing his essay in a separate form, and thus bringing it within the reach of every practising physician.

Army Medical Intelligence.

[From our Special Correspondent.]

WASHINGTON, D. C., Nov. 23, 1861.

MESSRS. EDITORS,—I am able, sooner than I expected, to resume my series of cases of army surgery and medicine.

The case of gun-shot wound of the arm and leg, of which I wrote you a short time since, has been discharged cured. Complete flexion of the arm and leg was the result, and, with the exception of two huge cicatrices, he is as well as before the wounds. The case of gunshot wound of the rectum still remains in the Hospital, but is gradually improving, each day bringing a firmer step and a brighter look to the patient.

My third case is that of a government teamster, A. McW., aged 27 years. On the morning of August 28, 1861, he was brought into the Hospital with terrible distension of the bladder, and in excessive agony. An instrument was used, but to no purpose, the urethra being firmly closed. Hot fomentations and hot baths failing to afford the least relief, an anæsthetic was administered, and perinæal section was performed. The operation occupied *three minutes.* A No. 8 steel bougie was introduced, and left, but during the hour the patient, in his semi-stupor, pulled the instrument out, which was again introduced and left in until Aug. 31st, when it was removed.

Sept. 3d.—No. 1 bougie could be passed into the bladder, but with much labor and pain. This, however, was done, and on September 6th, Nos. 3, 4 and 5 were passed. No bleeding followed. Sept. 10th, a No. 8 was passed. A little bleeding followed. The patient now became excessively annoyed by being obliged to pass his urine *thirty-five times* and upward during the day. The new urethra evidently had formed, and was granulating finely. Sept. 11th, one ounce of tepid water, with a drachm of sweet oil, was injected into the bladder, and on the 13th four ounces of a lotion of lead and opium, with a little oil, was injected. This operation was repeated on the 15th, and the result on the 17th was that the patient passed urine but *nine times* during the twenty-four hours. Here was a marked and decided improvement, consequent upon the simple remedy of injecting the bladder. And here let me take occasion to say that I have seen no less than seven cases completely *cured* by this simple yet most beneficial treatment. The patient got along finely till Oct. 6th, when a No. 12 bougie could be introduced into the bladder with ease. But a new trouble now arose. We found that in addition to his having had gonorrhœa three months previous to admittance, he had also had syphilis. His throat became sore, a large bubo appeared in the right groin, and pains in the limbs tortured him by day and night. His treatment for this consisted of potass. iodid., ʒiij. ; tinct. cinch. co., ℥iv. M. Mag. coch. ter die. Also, ol. morrhuæ and whiskey daily. A No. 12 bougie is introduced every third day without difficulty.

Oct. 19th.—Throat and limbs better, but syphilitic iritis is present. Large nodes appear over the whole surface of both tibiæ. He now has plain diet, and a fumigation of cinnabar gr. xiii. daily succeeds admirably.

Oct. 31st.—Cinnabar has produced an effect on the gums and mouth, and has been discontinued, but the nodes are much less in size and pain, and the iritis has entirely disappeared. His water passes in a steady stream, and the bubo is still very large, but hard and painful.

Nov. 17th.—Patient discharged. Passes urine as in health. Stricture entirely cured. Bubo opened and healed. Nodes less painful. Throat well, and patient is strong and able to resume his labors. H.

Selections from Medical Journals.

Paracentesis of the Stomach practised with Success in cases of acute Tympanitis. By Dr. Olivieri.—We translate from the *Gazette des Hopitaux*, where it is quoted from the *Gazette Médicale de Lyon*, the following article :—

> The disease for which M. Olivieri has employed paracentesis of the stomach is a special form of tympanitis which he has seen with considerable frequency in Bolivia, at Cochamba, where for three years he has held the place of principal physician to the hospital San-Juan-de-Dioz.
>
> The attack of this disorder, which always carries off the patient in twenty-four or thirty hours unless proper treatment is employed, is always sudden.—The belly from the first, says Dr. Olivieri, is hard, tense and full, at first unequal, afterwards uniform. On percussion, it sounds like a drum. The patient feels a continual desire to raise wind. There is obstinate constipation, the small pulse characteristic of grave abdominal disease, extreme difficulty of breathing, which increases every moment, face pale and hippocratic; the excessive size of the belly increases the dyspnœa, and a suffocating anxiety precedes death by some hours.
>
> Warned by two fatal cases of the insufficiency of the treatment usually employed, M. Olivieri had recourse, in a third case, to paracentesis, made by means of an ordinary trochar in the middle of a line drawn from the umbilicus to the xyphoid cartilage. This patient died, and the autopsy gave M. Olivieri an opportunity to see that none of the contents of the stomach had escaped into the abdominal cavity. The stomach was so distended that it reached down to the bladder (this meteorism was subsequent to the operation) ; it contained a great quantity of fœtid gas, and more than two quarts of a frothy liquid in an advanced stage of fermentation. The whole intestinal tube was also distended with gas.
>
> In all subsequent cases, M. Olivieri had recourse, at the commencement of the attack, to paracentesis, followed by energetic purgatives. Out of twenty cases, eight were perfectly cured in about three weeks, without the occurrence in any of them of inflammatory symptoms of any importance. The others died, says the author, because they were brought to the hospital in the last stages of the disease.
>
> The etiology of this disease is curious. M. Olivieri has only seen it in Bolivia and among the natives, whose food is almost exclusively vegetable (maize, potatoes and cocoa), and who use in excess *chica*, a liquor obtained by the fermentation of maize in water. It often happens that the process of fermentation is not complete at the time when they drink the chica ; it is then continued in the stomach, where it is favored also by the presence of a large quantity of vegetable matter, and in this process a large quantity of gas is evolved, which distends the stomach and intestines. A form of tympanitis is the result, very similar to that which is well known in the bovine race.

We will add, that perhaps the early use of emetics, and subsequently of powdered charcoal as an absorbent of the gas, might have obviated the necessity of employing paracentesis. We translate the above on account of the peculiarity of the disease and the impunity with which the stomach was punctured in so many instances.

Reproduction of Bone.—Dr. Cooper, the Editor of the *San Francisco Medical Press*, after quoting a case in which the right side of the inferior maxillary bone was almost entirely restored, after having been exsected, together with the articular condyle, remarks as follows upon this remarkable effort of nature :—

> The recorded cases in which reproduction of the bone has taken place are rapidly accumulating. It would appear that surgeons have lost nearly all their former fear of there being a want of reproductive power sufficient for the replac-

ing of bone when lost. But practitioners were very slow in adopting this opinion, until the evidence became overwhelming, though it would be difficult to tell why they were so slow in believing. Reasoning *a priori*, why should a physiologist hesitate to believe that lost bone would be readily reproduced?

It is a well-known law in physiology that, with few exceptions, nature tends to reproduce the same substance as that lost, differing only slightly in texture, let the substance be lost where it may. Thus, when muscle is lost to any reasonable extent, it will be reproduced, not with the same muscular contractility, but it will always have a little more of tendon-like hardness about it. Tendon and ligament are both readily reproduced the same as the original, except both are harder. Cartilage is sometimes reproduced in young children as cartilage of more than ordinary hardness, but in after years it is reproduced as bone. When bone itself is reproduced, it is still harder than the bone occupying its place before. It is, in fact, of almost ivory hardness, in many instances. When it embraces the articular extremities, we often find the ligaments and synovial membranes attached directly to the bony structure, instead of the cartilaginous envelope found in the natural state. All the softer structures tend to bone in old age, and, even in early life, bone is often found where none existed, as the result of morbid action. And, occasionally, the same thing occurs where there is no diseased action, the formation of bone, in such cases, being the result of much use. *Example*—The sesamoid bones, formed in the tendons of the great toes, where there is no periosteum to aid in their formation.

Reasoning from these premises, we early adopted the practice of taking out whole sections of the long bones, instead of amputating, even in the most extensive diseases of this structure, especially when occurring in bones of the fore-arm and leg. Even in diseases of the femur and humerus, we have, in several instances, removed entire sections, with the most happy results, a reproduction taking place with very little shortening of the limb. But, in all these cases, the periosteum was preserved, and where this substance can be left, we would not hesitate to remove the entire body of either of these bones, with the fullest confidence in the efficiency of nature in reproducing the requisite amount of bone. This often occurs when the periosteum is thickened to four or six times its natural condition, and the case terminates well, thus showing that an unhealthy periosteum is capable of producing healthy bone.

This we have noticed in many instances, and it has been verified positively, in one case, in which the abnormally thickened periosteum had reproduced healthy bone, and had itself resumed a healthy condition, under the greatest possible disadvantages. It is the case of James Gilmore, with which many of our medical readers of this city are familiar. Five inches of the lower end of the femur, including the condyles, were removed. The periosteum was about half an inch in thickness, and had but slight attachments to the bone, even at the point of insertion of the abductor magnus muscle, so that it was readily detached from the bone, as far up as the disease continued.

The limb being kept extended, the bone was rapidly reproduced, although, owing to a vitiated state of the constitution, the tibia and fibula became diseased, and, in twelve months after the operation, we were compelled to amputate the limb, purulent matter having been burrowing among nearly all the muscles of the thigh, until hectic set in, threatening a fatal termination. Notwithstanding purulent matter was burrowing all around the newly forming bone, it was fully developed, and as healthy as any bone, and of unusual solidity. The specimen is now in the Pacific Clinical Infirmary, where medical gentlemen are respectfully invited to call and see it. The reproduced lateral capsular and crucial ligaments are pretty well developed, and are adherent to the bony structures, in their usual places, though no part of the upper extremity of the tibia is more than a thin shell for several inches, the interior being completely disorganized.

From the favorable results of many similar cases in which an amputation had not to be performed, but where the periosteum was found thickened in the same manner, and the patient afterwards recovered completely, we are convinced that the periosteum will, generally, become healthy after the exsection of the diseased bone. The change of action of the periosteum, produced by the operation, will often, and, perhaps, generally, be sufficient to restore it to a healthy condition.

These remarks have been lengthened to a greater extent than was at first intended, but the importance of the subject is our apology. The entire subject of the reproduction of bone is in its infancy. Surgeons are just beginning to awaken to its importance. Much good will result from it, and those in active practice should always publish the results of their experience upon the subject.

PHYSIOLOGICAL ACTION OF CERTAIN REMEDIES UPON THE HEART.—The following is an extract from the letter of a Paris Correspondent to the *London Lancet.*

An interesting communication was made to the Academy of Sciences at its last meeting, by MM. Dybrowski and Pelikan, relative to the mode of action of certain poisons upon the heart. The experiments were made upon frogs, and the poisons (upas antiar, tanghinia venenifera, digitalis, and green hellebore) were either administered by the mouth or inserted under the skin in various parts. The results were constantly the same: the heart's action was arrested, although the nervous irritability and voluntary muscular power of the animal persisted for a considerable time. The first effect of these poisons was in all cases the same—namely, that of paralyzing the heart—whether inserted under the skin or given by the mouth. The average duration of the heart's action after the exhibition of the poison was, in the case of the upas, tanghinia, and hellebore, ten minutes; and in that of the digitaline, from ten to twenty. The action of the ventricle in the frog was invariably found to be arrested in systole; it was strongly contracted, pale, and quite empty, whilst the auricle was distended and gorged with blood. The contractions of the heart were sometimes accelerated at the commencement of the experiment, at other times they were less frequent from the beginning. The paralysis, or rather arrest of motion, in the case of the ventricle is sudden after a certain period in the diminution of the pulsations; that of the auricle is gradual and almost imperceptible—its contractions outliving those of the ventricle by some minutes. MM. Dybrowski and Pelikan have proved by experiment that these poisons exercise their deleterious influence upon the heart independently of the cerebro-spinal system, and that in those animals in which the medulla oblongata and pneumogastric nerves have been previously destroyed or divided, the toxical effects of these agents were equally manifest. They therefore consider that the action of these poisons is due to a special influence upon the nervous elements of the heart, or else upon two systems of nervous apparatus—the one presiding over the movements of the heart, the other possessing the office of regulating or diminishing these movements.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON: THURSDAY, NOVEMBER 28, 1861.

WE have received a communication from a highly esteemed correspondent, inquiring whether we agree with the opinions expressed in a recent notice published in our pages of Dr. C. T. Jackson's book on Etherization; and declaring that silence on our part must imply that we give the weight of the authority of the JOURNAL to Dr. Jackson's side of the controversy. Now we had not supposed that anything we might have said or left unsaid on this vexed question could awaken sufficient interest to revive a controversy which, it was to be presumed, had long ago died a natural death. We took it for granted that the question as to the merits of the two claimants to the discovery of the anæsthetic properties of ether, had long been settled in the mind of every physician in the land, and that again to stir up the waters of strife after so long a period of repose, would require a longer stick than we flattered ourselves capable of handling. It is true, that a perfect accordance of opinion on this subject may not exist, but we have little faith that such ever will. There are a thousand circumstances that influence the mind in the decision of such a question, and men equally honest, and equally wise, will differ in matters of far higher importance than this upon which so much has been said and written. We do not, therefore, see why the opinion incidentally expressed in the notice of the book above referred to, should be the means of calling forth remark. So far as ourselves are concerned, we do not conceive that the opinion of the Editors on this subject can have any more weight than that of the same number of the profession not associated with the Journal, and we therefore do not think it worth while to launch anew into this sea of doubts and contradictory statements by its proclamation at this time. We will merely say that a book notice, signed by the initials of the author, does not in our judgment imply coincidence of opinion on the part of the Editors. That such notices should contain opinions differing even widely from those, it may be, of a majority of the profession, should not, we think, exclude them, unless they contain statements manifestly false.

Messrs. Editors,—In your issue of Nov. 7th, is a communication signed "Radical," in which the writer states that *the most able physicians, accoucheurs and surgeons of Boston do advise, meet and consult with well-known homœopathists.* The charge is startling, and the information is new to very many of the readers of the Journal, and of the Fellows of the Mass. Medical Society; and, if true, is serious and should be looked up. It is in violation of the eighth by-law, which says, "And it shall be disreputable for any Fellow to advise or consult with any such irregular practitioner, or in any way to abet or assist him as a practitioner of medicine or surgery."

The matter is serious. Our State Society suffers in the estimation of the profession elsewhere, because it retains on its list of members the names of homœopathic practitioners. The fact that we are crippled in action by our legal charter, is not so well known. But if *Boston* physicians and surgeons, men, it is to be presumed, standing high in the profession, do thus violate our Society's laws and the laws of the American Medical Association, they bring additional disrepute upon the profession of the State, and a special stain upon the profession of Boston.

It is presumed that your correspondent, "Radical," has positive knowledge, or that he would not venture such an assertion. It is, then, his duty to make complaint, according to By-Law 31 of the Mass. Medical Society, and have offenders duly arraigned. The very positive and public nature of his allegation absolutely requires it, that the innocent among the city practitioners may be relieved of the imputation which now rests upon them, and the profession at large be vindicated from the reproach which it must otherwise suffer. Justitia.

Commencement at Berkshire Medical Institution.—We have received a letter from a correspondent, "Solon," from which we learn that Tuesday, the 19th, was Commencement day at this Institution. The writer speaks with great enthusiasm of the natural beauties of Pittsfield, and also in very complimentary terms of the new Medical College. This has been built, he states, at a cost of some $30,000, and is admirably located. Its exterior design and internal arrangements seem to be everything which could be wished. The graduating class numbered fifteen, and passed the ordeal of a rigid examination in a manner which reflected the highest credit upon themselves and their teachers.

The Alumni celebrated their anniversary by listening to an address, by whom is not stated. The graduates were also addressed in a very able manner by Prof. Chadbourn, of Williams College. The exercises of the day concluded with a dinner at the Berkshire Hotel, of which the Faculty, the Trustees, the graduates, the members of the Berkshire Medical Society, and a large number of invited guests partook. The festival throughout appears to have been most agreeable and satisfactory to all concerned.

We have received from the publisher, Mr. W. A. Townsend, of New York, the Physician's Hand-book of Practice, by Dr. William Elmer. This little annual is now too well known to need any special commendation from us. In addition to its convenience as a memorandum book of daily practice, the amount of valuable information it contains in such a small compass is really remarkable.

Deaths of Distinguished Physicians.—Among the recent deaths announced are those of Dr. James Wm. Cusack, of Dublin, at the age of 74, and of the celebrated Isidore Geoffroy St. Hilaire, who in the department of anatomy and physiology is said to have been second to no man living or dead, except his own father. He died on the 9th inst., at the age of 57.

VITAL STATISTICS OF BOSTON.

For the Week ending Saturday, November 23d, 1861.

DEATHS.

	Males.	*Females*	*Total*
Deaths during the week,	39	36	75
Average Mortality of the corresponding weeks of the ten years, 1851–1861,	36.3	37.2	73.5
Average corrected to increased population,	..	..	82
Deaths of persons above 90,	..	..	..

Mortality from Prevailing Diseases.

Phthisis.	Chol. Inf.	Croup.	Scar. Fev.	Pneumonia.	Variola.	Dysentery.	Typ. Fev.	Diphtheria.
13	0	1	2	11	0	2	1	0

METEOROLOGY.

From Observations taken at the Observatory of Harvard College.—For the week ending Nov. 16th.

Mean height of Barometer,	29.831	Highest point of Thermometer,	53.0
Highest point of Barometer,	30.152	Lowest point of Thermometer,	29.0
Lowest point of Barometer,	29.380	General direction of Wind,	W.N.W.
Mean Temperature,	36.8	Am't of Rain (in inches)	0.72

Books Received.—Lectures on the Diseases of Women. By Charles West, M.D., &c. Philadelphia: Blanchard and Lea. (From Brown & Taggard.)

Married,—In Philadelphia, Nov. 12th, Thomas George Morton, M.D., to Annie J., daughter of Dr. Thomas S. Kirkbride, both of that city.

Died,—At Val-de-Grace, France, at the age of 46, M. Scrive, Surgeon-General of the French Army during the Crimean War.

Deaths in Boston for the week ending Saturday noon, November 23d, 75. Males, 39—Females, 36.—Accident, 1—apoplexy, 6—disease of the bladder, 1—congestion of the brain, 1—disease of the brain, 3—inflammation of the brain, 1—bronchitis, 2—cancer (of the breast), 1—cholera, 1—consumption, 13—croup, 1—debility, 2—dropsy, 3—dropsy of the brain, 2—dysentery, 2—scarlet fever, 2—typhoid fever, 1—gastritis, 1—hæmorrhage, 2—infantile disease, 5—disease of the liver, 1—congestion of the lungs, 2—inflammation of the lungs, 11—old age, 1—pleurisy, 1—puerperal disease, 1—suffocation, 1—thrush, 1—unknown, 4—whooping cough, 1.

Under 5 years of age, 31—between 5 and 20 years, 6—between 20 and 40 years, 13—between 40 and 60 years, 15—above 60 years, 10. Born in the United States, 54—Ireland, 16—other places, 5.